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11 October 1977

Dear Josh:

You are right, I haven't been doing much about the Tatum piece. Since last April all of my time and energy have been spent on getting the origin of maize finished up--planning, planting, tending, pollinating, harvesting, etc., and have had little energy for anything else.

My other difficulty is that I don't see at all clearly how to do the piece on Ed. Aside from our collaboration on the science project, which was wholly satisfactory and highly rewarding, we were never close in other respects. I hope you can do better on the latter.

I am sure about the statement in my recollections piece that the approach came to me in one of Ed's lectures on Nils Fries' determination of growth requirements of various filamentous fungi. I believe it was the first time Ed gave that course, in which case it ought to be easy to check the Stanford records.

It was clearly on the basis of the Fries work that it occurred to me that if we could work out the nutritional requirements of Neurospora, a far more productive approach to the relation of genes to enzymes would be available to us. As you know, at that time biotin had just become available in a concentrate sufficient for our purpose. From then on it was clear we were on the right track.

As for the search for recombinants in E. coli, I had always assumed that you and Ed had available the mutants Ed's masters degree student, Gray, had isolated and identified as to requirements. From your latest note, I gather this was not the case--that you might not even have known of them, or if you did, did not make use of them. I am very clear about suggesting to Tatum the Neurospora approach to bacteria, for I was well aware of the Lincoln-Gowen paper.

Where do you go from here? I hope you'll see the light more clearly than I, and that you'll take the initiative.

I have now received copies of all the pieces on Ed for the Rockefeller University Service and am sending you Xerox copies of those and some other material I have.

As for the MacElroy recollection: It doesn't seem inconsistent to me in the time sequence of our Neurospora approach. I don't have written records but since our first Neurospora paper was in the October 1941 issue of PNAS, we must have got well started on the research by the summer of 1940 or earlier. Collecting stocks, determining nutritional requirements and producing of appropriate mutants could hardly have been done on a shorter time.

All the best,

Beets